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Yale School of Organization and Management

DOING RESEARCH THAT MAKES A DIFFERENCE

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Technical Report #3

DOING RESEARCH THAT MAKES A DIFFERENCE¹

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In concluding a retrospective review of his research on purposive behavior, E. C. Tolman wrote:

I started out, as I indicated in the introduction, with considerable uneasiness. I felt that my so-called system was outdated, and that it was a waste of time to try to rehash it and that it would be pretentious now to seek to make it fit any accepted set of prescriptions laid down by the philosophy of science. I have to confess, however, that as I have gone along I have become again more and more involved in it; though I still realize its many weak points. The system may well not stand up to any final canons of scientific procedure. But I do not much care. I have liked to think about psychology in ways that have proved congenial to me. Since all the sciences, and especially psychology, are still immersed in such tremendous realms of the uncertain and the unknown, the best that any individual scientist, especially any psychologist, can do seems to be to follow his own gleam and his own bent, however inadequate they may be. In fact, I suppose that actually this is what we all do. In the end, the only sure criterion is to have fun. And I have had fun. (Tolman, 1959, p. 152).

This quotation, printed and framed, was given to me by Neil Vidmar when I received my doctorate in social psychology in 1966. Neil and I had been in school together throughout our undergraduate and graduate years, and we agreed that Tolman's criterion was a good one for a psychologist to keep in mind while pursuing a career of research and scholarship.

Neil's gift is still on my wall (he had to order 25 copies to get the printer to produce one, so I always have a fresh copy), and I still believe what it says. Moreover, it is my strong (albeit undocumented) impression that

¹ This paper was prepared as a stimulus for discussion at a conference on "Doing Research That is Useful for Theory and Practice," held at the University of Southern California in November, 1983. Its preparation was supported in part by a contract from the Office of Naval Research (Organizational Effectiveness Research Program, Contract No. 00014-80-C-0555 to Yale University). Some of the material included is adapted from the author's chapter "Psychological contributions to organizational productivity" in A. P. Brief (Ed.), Research on productivity (New York: Praeger, in press).

most of us do our best work when it feels more like play than like toil.

So when a graduate student asks me what would be a "good thing to study," my answer invariably is something enormously unhelpful like "whatever it pleases you to study." This is not always believed, because I teach in an organizational behavior program located in a management school, and the presumption that research should inform managerial action permeates the place. But I'm deadly serious: I strongly prefer to see a student do first-rate scholarship that has uncertain relevance for action than second-rate work that is immediately applicable to some organizational problem.

For this reason, I approach our conference with a measure of ambivalence. On the one hand, what pleases me, what I find the most fun to do, is to wrestle with problems that offer both interesting conceptual challenges and the possibility of improving the effectiveness of social systems. So I am personally engaged by the topic of our conference, and pleased to have been invited. On the other hand, I would be dismayed if we were to find ourselves talking, even implicitly, as if the only worthwhile research in organizational behavior were that which contributes simultaneously to theory and to practice.

I say all of the above because this essay calls into question many well-accepted dicta about theory and method in organizational behavior. It describes what I think I have learned over the years in attempting to simultaneously build theory and improve practice, but it feels as if I am turning my back on big hunks of my training as a social scientist. And while I am convinced that the lessons I have learned make sense for me, for the kind of research I choose to do, these lessons may have little relevance for basic research in social science that makes no presumption of applicability.

Background

To provide a context for interpreting my comments, let me say a few things about the phenomena I study and the kinds of theories I like to build. I have, for more than fifteen years, been attempting to understand the factors that influence how people do work, and to frame that understanding in a way that invites constructive change in how work is structured and managed. My dissertation was on small group performance, and that topic has provided fun and frustration in approximately equal measures ever since. I also have spent some time studying individual motivation and performance, with special emphasis on the design of the tasks people do at work. And, recently, I have been attempting to learn how organizations can become more effective by fostering and supporting greater self-management on the part of organization members.

The notion of "performance effectiveness" is common to the several themes in my research. Since I intend to spend the rest of this essay discussing strategies for generating usable research and theory about performance effectiveness, let me take a few paragraphs to explain exactly what I mean by the concept.

I define an individual, group or organization as carrying out work effectively if the following three criteria are met:

1. The productive output of the performing unit exceeds the minimum standards of quantity and quality of the people who receive, review and/or use that output. There is no unidimensional, objective criterion of performance effectiveness in most organizational settings--and even when there is, what happens to a performing unit usually depends far more on others' assessments of the output than it does on any objective performance measure. So it is

necessary to pay attention to the evaluations made by those who have a stake in the group's output--even though this may require us to deal with multiple and conflicting assessments of how well a unit is performing.

2. The process of carrying out the work enhances the capability of the performing unit (be it an individual, a group or an organization) to do competent work in the future. Organizations are not single-shot systems, and the way any single task is carried out can strongly affect the capability of a performing unit to accomplish subsequent tasks. A unit that "burns itself up" in the process of doing a task is not viewed as effective--even if its product in that specific instance is fully acceptable.

3. The work experience contributes to the growth and personal satisfaction of the persons who do the work. Sometimes the process of carrying out a piece of work serves mainly to block the personal development of individual performers, or to frustrate satisfaction of their personal needs. In such cases, the costs borne by individuals in generating the work product are sufficiently high that the performing unit is not viewed as effective--even if its product is fully acceptable.

This way of thinking about performance effectiveness, then, involves far more than simply counting outputs that meet a predetermined quality standard. The use of client evaluations of work products, for example, shifts primary control over the choice of assessment standards from researchers to those who use and are affected by what is produced. And the social and personal components of the criterion are explicitly normative in asserting that certain group and individual outcomes are generally to be preferred over others. These are relatively non-traditional ways of thinking about performance effectiveness, and they impose on the researcher both a greater measurement

challenge and a higher data collection workload than is usually encountered in assessing work outcomes.

Yet the criteria themselves are modest. All that is required to exceed minimum standards for effectiveness is output judged by those who receive it to be more than acceptable, a performing unit that winds up its work more competent than when it started, and performers who are more satisfied than they are frustrated by what has transpired. The challenge in my work has been to develop ways of understanding, designing and managing performing units that increase the chances these modest criteria can be met. And what I have to say about research strategy in this essay is based on my history of attempting to make some progress on this general issue.

I will frame my thoughts as a series of assertions, each of which summarizes something I think I have learned about what is required to develop usable research and theory about performance effectiveness as I have defined the concept. Each assertion begins with a negative learning, something I have found not to work as well as I once hoped and expected. Then I will raise some alternative ways of proceeding with research that may circumvent the difficulty--including some strategies I am using in my current research on team effectiveness, and others that remain to be explored in the future.

ASSERTION ONE:

Laboratory research methods are not much help in developing practical theory about performance effectiveness--but for reasons different than those we usually cite when complaining about laboratory studies.

It sometimes is argued that laboratory research, because of the inherent artificiality of the situation, is not useful for understanding organizational phenomena. I disagree. If the phenomena addressed by some organizational theory are actually created in the laboratory, and if appropriate choices are

made about relevant variables (i.e., what variables to manipulate, to control, to measure, and to ignore) then laboratory research can provide powerful tests of conceptual propositions, including propositions about behavior in organizations (Runkel & McGrath, 1972, Weick, 1965). Moreover, certain research objectives (e.g., discovering what can occur rather than documenting what usually occurs) sometimes can be better pursued in the laboratory than in more "realistic" field settings (Mook, 1983).

There is, however, a real risk in studying performance effectiveness in the laboratory--a generally unrecognized risk that has less to do with the absence of mundane realism in the laboratory than with the kinds of variables about which one can reasonably expect to learn using laboratory methods.

Consider, as a case in point, research on small group behavior and performance. Laboratory studies of small groups tend to focus on individual, interpersonal and group level variables, holding constant (or ignoring) the relationship between groups and the contexts in which they operate. Indeed, laboratory researchers learn quickly that one had better control variables such as task characteristics, experimenter-subject relationships, reward system properties, and the demand characteristics of the research setting.² Not to do so is to invite these variables to overwhelm the more subtle intra- or inter-personal phenomena one is attempting to study.

² This does not mean that contextual forces are absent. They are present in the person of the experimenter: it is he or she who picks the place where the study will be conducted, recruits the subjects and forms them into groups, selects and assigns the group task, decides what rewards will be available and administers them, provides groups with the information and resources they need to do their work, and establishes the basic norms of conduct that guide behavior in the setting. In essence, the experimenter creates an organization that serves as the context of the group, serves as the top management of that organization, and (if expert in his or her role) makes sure that contextual factors are as nearly the same for all groups as is possible.



But what if contextual and environmental variables should happen to be among the most powerful influences on group performance? This is not an unreasonable possibility (e.g., Hackman, *in press*; Pfeffer & Salancik, 1978). It just may be the case that, in the interest of good experimental practice, some of the variables that most strongly affect group behavior and productivity are usually fixed at constant levels in laboratory research, thereby ruling out any possibility of learning about their effects. By contrast, these same features of the group and its external relations receive special attention in many state-of-the-art action projects in which self-managing work teams are created in organizations (e.g., Poza & Markus, 1980).

While this example is taken from small group research, contextual and environmental variables typically are ignored or fixed at constant levels in laboratory experiments on other performance-relevant phenomena as well. This is readily understandable, because it is extraordinarily difficult to manipulate such factors well in the laboratory, as researchers who have attempted to create temporary but real organizations for research purposes will attest. But the example does raise questions about the usefulness of laboratory methods in research on performance effectiveness.

The liabilities of the experimental laboratory for developing practical theory, then, have little to do with the artificiality of the setting per se, or with the limited ecological validity of the setting (Berkowitz & Donnerstein, 1982). The problem, instead, is that those variables that lend themselves to study in the laboratory may be less important in influencing performance effectiveness than those that are difficult or impossible to deal with in that research setting.

What research strategies might be preferable to the laboratory for studying the relationship between a performing unit and its organizational/environmental context? Neither organizational sociologists (who are interested in the links between organizations as total systems and their environments) nor we organizational psychologists (who tend to discard the complexity, and the guts, of contextual phenomena to make them researchable using standard methods) have made a great deal of headway on this question.³

Clearly, understanding contextual and environmental relations requires that there be substantial variation in the features of the performance situation. This suggests that a field setting may be called for--but merely conducting the research in a "real" organization does not automatically take care of things. One can no more learn about contextual influences in a single, homogeneous unit of an organization during a period of relative stability than one can in a laboratory, since all members of that unit work within essentially the same context.⁴

Particularly inviting are settings where organizational changes are taking place. The changes may involve planned alterations of the work context, or they may be responses to a changing external environment. In either case, there is variation in the phenomena of interest, and therefore study of those phenomena is possible. Another alternative is to gather data from a number of different performance situations, and conduct comparative

³ Both sociologists and psychologists have, however, shown an interest in organizational design, an important question but a different one.

⁴ This lesson was learned, or should have been, by those job design researchers who attempted to assess the relationship between job characteristics and work outcomes by studying their correlations in a single organizational unit where all employees performed the same basic job.

analyses. I have used these strategies in my current research on work group effectiveness, and with each of them have found it necessary both to use multiple data collection methods (i.e., observational, interview, survey, and archival techniques) and to collect data from multiple perspectives. Just as there is no one method that can adequately capture the complexity of contextual influences on group behavior, neither is there any single accurate description of the context or how it operates. Because there are many separate (and not necessarily correlated) truths about the context of a group, any reasonably complete understanding of contextual influences requires that they be examined from multiple perspectives using a variety of measurement devices.

A seemingly very attractive research strategy would be to conduct a field experiment (or quasi-experiment) on the impact of the work context. If appropriately designed, a field experiment could provide access to the contextual variables of interest, the opportunity to manipulate those variables to create the needed variation, and researcher control over factors (such as assignment of participants to conditions) that otherwise might confound the results. Unfortunately, as will be seen below, field experiments, for all their advantages, also have some serious problems as devices for developing usable knowledge about performance effectiveness.

ASSERTION TWO

The field experiment may be a fundamentally inappropriate device for use in developing practical theory about performance effectiveness.

Several years ago Clayton Alderfer and I wrote a proposal for a field experiment to compare team building and job design as points of intervention for initiating performance-relevant organizational changes. The proposal was

well-received, and we were encouraged by all who read it to proceed to find a site and conduct the experiment. Over the next two years, Clay and I spent a great deal of time together--attending meetings, hearing about impending reorganizations, watching managers with whom we were about to contract for the research be transferred, and reassuring each other that the project was worth the investment we were making in trying to get it underway. Eventually we decided it wasn't and we abandoned our plans.

Recently some colleagues and I did succeed in negotiating something almost as good--a decent quasi-experimental design in which a performance-relevant independent variable would be manipulated at different levels for different groups, and follow-up data would be collected longitudinally. This time the reorganization and managerial realignments occurred after the study had begun, and these events were supplemented by the dissolution of about half the groups we were studying..

Organizations do not hold still while we negotiate entry, make our intervention, and wait for an appropriate time to collect follow-up data. Although it took me too many years to learn this, it is not a surprising learning, and it simply attests to the difficulty of negotiating and executing field experimental research. It is not the point I want to make here.

The point is this: if we were able to successfully negotiate a field experiment, execute it, and gather follow-up data right on schedule, we would then need to worry about the external validity of the findings--their generalizability to other organizations. Why? Because any organization that could or would hold still long enough for such research to be done, and that would relinquish to researchers the level of control needed to run an experiment (e.g., determining how people are assigned to conditions, designing

the intervention and the measures, deciding when they will be administered, and so on) would be a pretty strange place, unlike the great majority of work organizations to which we would wish to generalize our findings.

So there are three ways to lose in field experiments on performance effectiveness: (1) we can fail to gain entry, (2) we can fail during execution, or (3) we can succeed in getting the study done just the way we wanted it done. Field experimental designs are not, I fear, a very good way to generate usable findings about performance in organizations. Not because organizations are not cooperative, or because researchers are incompetent, but because the field experimental model is inappropriate for such research.⁵

What, then, are some alternatives to field experimental designs? The usual response when this question comes up is to suggest that a quasi-experiment be conducted. Yet quasi-experimental designs that require researchers to have control over significant organizational interventions, or that need organizational realignments to be put on hold until after time series data have been collected, are subject to many of the same problems as true experimental designs.

Rather than continue trying to force the world to fit with the designs we know and know how to use, I suspect we need some innovative thinking about methodologies for studying productivity in organizations. Can we, for example, find ways to create mutually beneficial partnerships with

⁵ A well-documented case analysis that illustrates many of the built-in limitations of the field experimental model is provided by Blumberg and Pringle (1983). Titled "How control groups can cause lack of control in action research," their report describes what happened when a "good" experimental design was used to study the outcomes of a quality of worklife program in a coal mine. In essence, the research design prompted a number of unanticipated and unfortunate consequences (such as widespread conflict and dissension throughout the organization) which, in turn, contributed to the premature demise of the very program whose effects were being researched.



organizations, in which the researchers and organization members collaborate to learn about factors that influence individual and group performance?⁶

The researchers in such a partnership would bring some special expertise to it (e.g., regarding the construction of reliable measures whose validity can be assessed, or the invention of methodological strategies that allow for relatively unambiguous attributions of causality). Organizational representatives also would have much to contribute (e.g., regarding special constraints on what can be done in the organization, special opportunities for learning that may be coming up, issues that are of special importance to organization members, and concepts that have special meaning or history in the organizational culture).

By working together, researchers and organization members should be able to tailor the research to the special constraints and opportunities that exist in the social system. Moreover, since the aim of the research would be to generate learnings about real organizational concerns, both partners should be motivated to ensure that the data be trustworthy and meaningful and that inferences be logically sound and backed by data.

A research partnership requires a commitment by both partners to develop ways of learning that subordinate neither party's legitimate needs to those of the other, and to seek out and exploit opportunities for learning as they develop in the social system. This demands great sensitivity to questions of timing, creativity in finding ways to learn from events that have not been designed for learning purposes, and a willingness by the researcher to share control over the research process with people who are much more concerned with organizational needs than with the dicta of research methodology.

⁶ For ideas about how to do this, see Alderfer, Brown, Kaplan and Smith (in press) or Hakel, Sorcher, Beer and Moses (1982).

Collaborative, opportunistic research is demanding, often is frustrating, and altogether is a chancier enterprise than I personally find comfortable. It also offers a means to generate learnings that may not be obtainable in other ways, however, and it frees researchers from the burden (and ultimate futility) of trying to find organizations that will let them have the level of control they need to satisfy the stringent requirements of field experimentation.

ASSERTION THREE:

Searching for unitary causes of performance effectiveness can make it harder, not easier, to learn about the organizational conditions that foster good performance.

When something happens in an organization that improves productivity, managers are happy and psychologists are frustrated. "What actually caused the improvement?" we ask. And we begin to take apart the inevitably fuzzy and multi-faceted change, first conceptually, then often empirically--perhaps in a laboratory experiment that isolates the suspected cause, or using structural modelling techniques with survey data. We want to rule out as many possible explanations for the observed phenomenon as we can. We want to pin down the true causal agent.

Consider, for example, the review by Locke and his colleagues (Locke, Feren, McCaleb, Shaw, & Denny, 1980) comparing the relative efficacy of goal setting, compensation, participation, and job enrichment. This review provides an excellent comparative evaluation of the programs reviewed, and it is a valuable contribution to scholarly thought about behavior in organizations. But empirical studies and review articles that attempt to isolate unitary causes may not be of much help in generating theoretical propositions and research findings that can be used to improve performance.

Influences on performance do not come in separate, easily distinguishable packages. They come, instead, in complex tangles that often are as hard to straighten out as a backlash on a fishing reel. Indeed, to try to partial out and assess the causal effects of each piece of a multi-faceted organizational change may lead to the conclusion that nothing is responsible for an observed improvement in performance--each ingredient of the spicy stew loses its zest when studied separately from the others.

Teasing out the separate effects of various interventions does, of course, help us obtain a sense of how potent they are when isolated from other factors that may also enhance or depress performance. The problem arises from the fact that there are many ways to be productive at work, and even more ways to be nonproductive. If our attempts to understand what causes productive work behavior focus on single causes, we are unlikely to generate a coherent understanding of the phenomenon. There are simply too many ways to get there from here, and the different routes do not necessarily have the same causes.

Systems theorists call this aspect of organized endeavor "equifinality" (Katz & Kahn, 1978, p. 30). According to this principle, a social system can reach the same outcome from a variety of initial conditions and by a variety of means. Equifinality encourages us to view the management of work performance as essentially involving the creation of multiple conditions--conditions that support high productivity, but that also leave individuals and groups ample room to develop and implement their own ways of accomplishing the work within them.

The best way to improve performance, then, might be to alter several factors all at once, to create a "critical mass" of favorable conditions, and to deliberately foster redundancy among positive features of the performance

setting. Unfortunately, when one looks through the literature to see how scholars in organizational behavior think about and study performance phenomena, one sees theories and research paradigms that are conceptually clean and often elegant--but that provide little help in learning about messy, overdetermined organizational phenomena.

If performance outcomes are, in fact, overdetermined--that is, if they are products of multiple, non-independent factors whose influence depends in part on the fact that they are redundant, then we will have to find some new ways of construing and researching performance phenomena. The comfortable "X is a cause of Y, but their relationship is moderated by Z" kind of theorizing will have to go, for example. Moreover, several key assumptions of our powerful multivariate models, models designed specifically for analyzing causally complex phenomena, would be violated so badly that we could not use them for studies of influences on work performance (cf. James, Mulaik & Brett, 1982). Are there alternative approaches that might be adopted for studies of work performance, approaches that would fit better with the phenomena?

One possibility, which has received surprisingly little attention, would be to bring the case study out of the classroom and put it to work in scholarly pursuits. It is true that case studies, as traditionally prepared, may give too much credence to the interpretations favored by their authors. Selective emphasis of material, and decision-making about what data to include and exclude are real problems (although these problems are shared by writers of quantitative empirical studies to a far greater extent than we usually admit). Can we think of ways to present case studies that invite disconfirmation and tests of alternative interpretations? Would it be possible, for example, to carry out competing analyses for each interpretation

of a case that we generate--one that seeks to make the best case possible for the interpretation, and one intended to cast the greatest possible doubt on it? Would such an approach to case analysis and presentation foster learning by other scholars, and contribute to the accumulation of knowledge across cases studies? We are trying this kind of approach now in attempting to learn as much as we can from our detailed, descriptive analyses of task performing teams. And while it is too early to assess the ultimate efficacy of the approach, we certainly are learning a great deal in trying to use it.

Another possibility, heretofore used more by coroners, detectives and aircraft accident investigators than by scholars of organizations, is the "modus operandi" method (Scriven, 1974). If one can generate a list of the possible causes of some outcome or event, and has some knowledge about the special "signature" of each one, then it often is possible to use logical, historical, and micro-experimental techniques to disentangle the probable causes of that outcome--even when it is complexly determined or overdetermined. The modus operandi approach, which so far as I know has not been used in the study of work performance, provides an intriguing alternative to standard quasi-experimental and correlational studies of organizational phenomena.

Whatever the new devices we come up with for attempting to develop usable knowledge about overdetermined organizational phenomena, I suspect that they will involve thick, systematic description of those phenomena, and that they will require interpretations that cross traditional levels of analysis (i.e., that link individual, group, organizational and/or environmental variables). We may even see greater recognition of the value of multiple perspectives on the same data, from people in different groups with different "stakes" in how

those data are interpreted. What we will see less and less, I hope, are analyses of the causes of performance outcomes that isolate causal agents from the social systems in which they operate.

ASSERTION FOUR:

Contingency models of behavior in organizations are of little practical use in managing work performance.

Contingency theories of behavior in organizations typically hold that the relationship between some predictor variable (e.g., how a job is designed) and some outcome variable (e.g., quality of performance) depends on some third variable (e.g., a measure of the characteristics of the performer, or the attributes of the situation where the work is being done). Contingency theories contrast with universalistic models of behavior at work (i.e., those that posit that a given variable will operate in more or less the same way for all people and situations normally encountered in work organizations). Such theories have been much in vogue in industrial-organizational psychology in the last decade, and have been prominent in my own work.

Where do contingency models come from? Sometimes they are generated out of a researcher's desperation. Findings that were supposed to match other findings do not, and the researcher goes on a search for the reasons the expected replication did not occur. Such searches are almost always successful: a plausible explanation having to do with individual differences or situational attributes can be found for virtually any unexpected finding. Unfortunately, as Hunter, Schmidt and Jackson (1982) note, variation in findings across studies or samples often is the natural statistical result of small sample sizes, restricted range of variables, and/or unreliability of measures. Contingency theories based on such variation will not hold up when

properly tested. On the other hand, contingency models that are based on an in-depth understanding of the phenomena and thoughtful conceptual analysis can be quite helpful in sorting out complex phenomena. The normative model of leadership decision-making proposed by Vroom and Yetton (1973) is a good example of a conceptually sound contingency theory.

To assess the usefulness of a contingency model as a guide for organizational practice we must ask two questions. First, does the model predict the outcomes of interest more powerfully than simpler "main effect" models that address the same phenomena? And second, is the model framed in a way that makes it usable by practitioners in their work?

Unfortunately, the answer to both questions for contingency models having to do with work performance appears to be a qualified "No." While there are some exceptions, the general direction in research guided by contingency thinking has been to make more and more distinctions, and to add ever more conditions and qualifications to general propositions. The point of diminishing returns is reached soon: increments in explanatory power come more slowly than increases in model complexity.

Moreover, research in cognitive psychology casts doubt on our ability to process multiple contingencies in making decisions about our behavior (see, for example, Slovic, 1981). Indeed, one distinguished contingency theorist has even had a black box constructed to guide managerial decision making. The manager sets various switches in accord with the characteristics of the decision situation, pushes a button, and has electronically revealed the course of action that, according to the theory, should be followed. So far the theorist has chosen not to market the device (its construction was something of a light-hearted enterprise), but it nicely symbolizes the difficulty of using complex contingency theories as behavioral guides.

Are there alternatives to contingency models that would provide more powerful and practical conceptual tools for managing work performance? One intriguing lead is offered by the theory of multiple possibilities set forth by Tyler (1983). Whereas contingency theory assumes that if we knew the right moderating variables we would be able to predict and control behavior in virtually any situation, multiple possibility theory holds that such an aspiration is ill-conceived. Instead, the theory maintains, there are many possible outcomes that can emerge in any situation, and the particular outcome that is actualized is not completely determined by the causal factors that precede it. Thus, multiple possibility theory envisions a world with some "play" in the system, and it encourages attention to human choice as a factor that transforms multiple possibilities into single courses of action.

Multiple possibility theory nicely complements the system theorists' notion of equifinality discussed in the preceding section. Where equifinality alerts us to the fact that the same outcome can occur in response to many different causes, multiple possibility theory posits that the same cause can generate a variety of different outcomes. Taken together, the two notions call into question standard, stimulus-response models in which situational causes are tightly linked to behavioral effects--whether directly ("Introduce this management practice and performance will improve") or contingently ("...performance will improve, but only for certain kinds of people under certain circumstances").

If we were to take seriously the notions of equifinality and multiple possibilities, would that signal an abandonment of "scientific" approaches to understanding behavior in organizations? Not at all. But it would require that we generate qualitatively different kinds of scientific models of

organizational behavior, and that we invent some new methods for assessing the validity and usefulness of those models.

What kinds of theories, for example, would exploit rather than suppress the systemic context in which work is done? Can we envision performance models that deal explicitly with the ways that symbols, language and physical place affect both how people comprehend their workplaces and how they assign meaning to what happens within them? What would be the attributes of a performance model that would allow us to learn about self-reinforcing spirals of performance, illuminating how the choices people make at a given moment affect their capabilities for future performance--often resulting in well-performing units finding it easier to perform even better, while poor performers become ever poorer? What would be required of scientific models that are oriented more toward understanding the conditions and contexts that shape the choices people make about their behavior than toward pinning down the immediate, proximal causes of specific performance outcomes?

What kinds of methods would be needed to generate moving pictures of performance as it changes over time, rather than still pictures of what is happening at a given moment? How could we go about studying multiple, redundant influences on performance in ways that yield more than mere descriptions of what transpires in work organizations--that, instead, offer insight into the kinds of organizational conditions that foster and support excellence? What methods might be used to learn about factors that have powerful cumulative influences on work behavior, but whose effects are almost impossible to discern at any given moment in time?

In my view, these are substantial conceptual and methodological challenges, and certainly not ones for which I have ready answers. But if we

could begin to confront them, then we might find ourselves on the way toward the development of scientific models of work performance that are considerably more congruent with the realities of the social systems where work is done than the deterministic contingency models we presently favor. And, in the process, we just might generate some guidelines for managerial action that would be both powerful in affecting work performance and usable by people who design and manage work organizations.

Is the level of disaffection with current conceptual and research paradigms sufficiently high that organizational psychologists are likely to explore a radically different approach to their subject matter? I think not--even though there is real movement in related disciplines toward the development of alternative kinds of knowledge and ways of knowing (e.g., McGuire's (1983) contextualist theory of knowledge in social psychology). Indeed, I see in organizational behavior signs that at least some of us feel that the best remedy for the dis-ease we are now experiencing would be a return to more orthodox scientific models and methodologies. I remain hopeful, nonetheless, that at least a few of us will venture into the relatively uncharted territory I have tried to sketch here, and report back on what is found.

ASSERTION FIVE:

Evaluation research that assesses currently popular productivity improvement programs allows both managers and scholars to avoid addressing fundamental questions about how organizations are designed and managed.

How can one argue about the value of evaluation research in our field? The history of management is filled with various fads and fashions which, when subjected to empirical assessment, have proved to be of little value. And,

occasionally, research has shown that some management devices, appropriately used, can help improve work performance in organizations.

We have done MBO, job enrichment, T-groups, goal-setting, zero defects, brainstorming, and a multitude of others. Now we are examining newer programs, such as quality circles, quality of worklife programs, and gain sharing plans. Soon still others will emerge, and we will take a look at them. Part of the burden of being a social scientist interested in organizational performance, it seems, is that one must be ready to gather up one's methodological tools and pack off to evaluate the latest productivity improvement scheme. Although we sometimes risk losing a few consultant friends along the way, the work is important and ultimately constructive.

It also is insufficient, and a diversion from what we really ought to be doing if we aspire to research that has significant implications for organizational effectiveness. What bothers me is not what typical productivity improvement programs do, but what they do not do. Understandably, managers would like to obtain improvements in productivity with as little effort, anxiety, and disruption to standard organizational practices as possible. As a consequence, productivity improvement plans that gain easy acceptance by the management community tend to be those that do not call into question (a) the authority structure of the organization, (b) the core technology used by the organization in making its product or providing its service, or (c) fundamental managerial values and assumptions about how human resources are used in the organization and about the personal and financial rights of employees.

By studying only programs that are readily acceptable to management, we close off the opportunity to learn what might happen if some of management's

unquestioned "givens" were altered. Worse, we may unintentionally and implicitly support the notion that relatively modest, nonthreatening programs are the best that behavioral scientists have to offer. The result can be a continued collusion between ourselves and managers, an unstated agreement that the search for ways to improve work performance will not seriously address the possibility that the way work is designed, organized and managed in this society underutilizes and misuses human resources.

We obviously cannot study that which does not exist, so what are we to do if we harbor a suspicion (as I do) that many opportunities for improving performance effectiveness lie hidden in management's unexplored forbidden land? Three possibilities come to mind.

First, we can watch for occasions when unexpected or unintended changes in authority structures, technologies, and human resource strategies do occur, and be prepared to exploit the learning opportunities these occasions provide. When a crisis occurs, for example, an organization may temporarily operate in ways that management would find wholly unacceptable during normal operations. If we are present, prepared, and not already fully occupied evaluating the latest productivity program, we may be able to capitalize on such occurrences--and just might generate findings showing that "unacceptable" ways of operating actually result in improved performance effectiveness.

Second, we can seek out organizations that go about their business in ways that differ markedly from standard corporate practice. We have much to learn from public and nonprofit organizations, for example. And of special interest are work organizations that have chosen a deliberately democratic model of governance, such as worker cooperatives. Some of these organizations manage the productive work of the firm using interesting, nontraditional

structures and systems. They can serve as a kind of laboratory for examining the impact on performance effectiveness of ways of operating that are quite unlikely to appear spontaneously in more traditional businesses.

Finally, we can prepare ourselves to help create nontraditional organizational forms when opportunities present themselves, and carefully document what happens and what is learned in the process. The creation of new plants, for example, has provided some valuable opportunities to learn about alternative ways of improving productivity, even in corporations whose headquarters operate quite traditionally (Lawler, 1978). This option is, perhaps, the most engaging and promising alternative to evaluation research on productivity improvement programs. It is also the most challenging, in that it requires not only a model of the conditions that foster work effectiveness, but also a theory of action to guide the implementation and management of the innovative system (Argyris, 1980). It is hard to deal with "what is to be done" and "how and when should we do it" questions at the same time. It surely is worth the trouble to try.

Yet we may have to go even further. If we seek to do research that can have a significant impact on organizational performance, we may have to start dealing explicitly with the assumptions and values held by managers in the organizations where the research is conducted. And to ask managers to examine their unstated assumptions and values requires that we be aware of our own--and be willing to make them explicit. If our research is intended to generate knowledge useful in improving productivity, for example, then we must be prepared to assert that we believe improved productivity to be a positive outcome, something worth espousing and supporting.

One can, of course, take the contrary position, that research and practice aimed at productivity improvement are not desirable for this society at this time in history. But if we choose the view that higher productivity is beneficial, and involve ourselves in research or action intended to promote it, then it seems to me we are obligated to do that work as well, and with as much impact, as possible. And this will, on occasion, require us to confront managers directly about what is, and what is not, open to change in an organization.

This position can create some dilemmas for those of us who work in organizations where management is willing to accept from behavioral scientists only those contributions that do not call into question unstated assumptions about such matters as choice of technology, the distribution of authority, the allocation of gains realized from productive work, and the strategy of the firm in obtaining, developing and using human resources.

If our professional judgment is that such issues may be key to performance effectiveness in an organization, should we insist that they be addressed, and decline invitations to work on problems that divert attention from them? Or should we pitch in, try to find constructive things to do within existing constraints, and hope that opportunities for real contributions to productivity improvement will develop at some later time? Such choices are, obviously, much easier for an academic to make than they are for someone whose livelihood depends on keeping a job in the organization where the work is being done. But even academics, including myself, too often find reasons to defer discussions about organizational values and assumptions, and about what may be required if there is to be any real chance of achieving nontrivial gains in organizational effectiveness.

Should there be any misunderstanding, let me state that I am not advocating that we go on strike for greater impact. All I am suggesting is that we make explicit the values on which our work is based, that we assess the usefulness and the impact of what we do with respect to those values, and that we try to avoid getting ourselves into a position where our work unintentionally impedes progress toward the very ends we seek. Those of us who study performance in organizations, or who attempt to promote it, should never have to respond, when asked why we are doing what we are doing, "I had no choice." As professionals, we always have a choice.

CONCLUSION

We just may be leaving the period of history when people write of the great promise of "applied behavioral science." Implicit in the whole of this essay is the view that it probably is futile to try to take the results of basic research in the social sciences and apply them intact to solving organizational problems. In fact, the reverse may be true: application-focussed research may be more useful in generating advances in basic knowledge than basic research is in generating applications. The history of experimental psychology, for example, shows that many of the most significant and fundamental conceptual problems in that field had their origins in work that initially was focussed specifically on solving real problems of human perception and cognition (Garner, 1972). It may be that the best way to generate advances in basic theory is to do research that seeks solutions to real problems, and to keep one's eyes open for fundamental conceptual issues as one proceeds.

In organizational behavior, this approach will require that our research be tailored to the special circumstances of the social systems in which we conduct our studies and to which our findings ultimately are intended to apply. The theory and method of the social science disciplines were not, by and large, designed for that task--and I believe them to be insufficient for it. Moreover, they have led us to study factors that have ripple rather than tidal effects on organizational phenomena. We need now to open ourselves to the larger organizational forces, and to develop conceptual models and research methods that address them directly--rather than persist with our well-worn traditional models, hoping that one day we will find places to apply them or, worse, that one day organizations will change to fit with them.

Such a re-orientation will require, for most of us, both a good measure of inventiveness and some fundamental changes in how we think about and go about our work. But it will not require us to abandon the core values that underlie scholarly work. Traditional conceptual values, such as parsimony and sound logic, are just as essential for research on real problems as they are for paradigm-driven studies of interest only to other academics. And traditional methodological values--such as the disconfirmability of findings, measures whose reliability and validity are publicly demonstrable, and means of inference that allow for relatively unambiguous and logically defensible attributions of causality--are, if anything, more critical for research whose findings can substantially affect people's lives than they are for paradigmatic research. These values are much to be cherished, and in my view they should be rigorously taught to each cohort of fledgling organizational scholars. They are the bedrock on which scholarly work in organizational behavior is done, and they provide a firm enough foundation to allow

structures to be built that are, if unconventional, more suitable for our phenomena than those previously built by other scholars for other purposes.

The re-orientation about which I have been speculating in this essay is not a simple undertaking, and (if my own experience in attempting to behave in accord with my words has any generality) it invokes no small measure of ambivalence, for at least two reasons. First, despite my intellectual confidence that new conceptual and methodological approaches are required in organizational behavior, experimenting with those approaches occasionally makes me feel as if my deviations from traditional ways of pursuing scientific values are somehow heretic and sinful. And second, when one is attempting to do something that one does not know how to do, and for which there are no ready models, failure is always a real possibility, and probably more likely than success. Such ambivalence tends to be accompanied by anxiety, which in turn can block intellectual work and make it hard to get anything done, let alone something new and possibly interesting. But ambivalence and anxiety also are reputed to be the precursors of creativity, so there is always hope that something worthwhile will, in fact, emerge if one sticks with it long enough.

These, then, are the kinds of questions and issues, both emotional and intellectual, that I am wrestling with these days as I continue to try to develop practical theories of individual and group performance effectiveness. I am finding the challenges--to my imagination and to my courage--substantial. I'm awfully glad I have Tolman's quotation mounted on my wall, because sometimes I need a little reminder about how much fun I must be having.

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